

Interactive comment on “The contribution of respiration in tree-stems to the Dole Effect” by A. Angert et al.

Anonymous Referee #2

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This is an interesting easy to read paper addressing a component of the global Dole effect in atmospheric O₂ for which we did not have specific information (18O discrimination of tree stem respiration). The Dole effect is a powerful signal which is underutilized and any effort in advancing its use is well worth publication and will advance the field. The paper also uses a unique measurement approach.

The weaker aspects of the paper are first, that it is based on a rather limited survey, in terms of sites and species, for modifying the global scale discrimination value, δ , and the global Dole effect.

Second, in parts it gives the sense of a Response to the paper of Severinghaus et al 2009 that also tried to improve the global Dole effect estimates. Interestingly, Severinghaus et al. used a previous paper of Angert et al reporting a limited soil survey

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that argues for modified soil δ in the global Dole effect. And now, the authors use another limited survey of stems respiration and suggest that perhaps the earlier correction made by Severinghaus based on the earlier Angert paper... may be incorrect.

And so, while I think the measurements that Angert et al are doing are highly valuable, the conclusions also demonstrate the risks and dangers of partial and limited surveys. It seems therefore that in publishing such data the authors must make a much more serious effort to provide a wider perspective indicating the preliminary nature and the dangers in building up a theory on this.

I therefore suggest this could be published after major revision and additional review.

Some specific comments:

Abstract: L4, isotopic discrimination

Methods: Israeli site with no met data as given to the Peruvian site. Chambers used in Peru need diagram. Not clear Checking CO₂ for leaks? No indication of flow through capacity, use of IRGA? Other means? Chamber in Peru was incubated for 10 days. No info for sampling detail in Israel Model: assumption of little dissolution or degassing does not say much about possible extent 'isotopic exchange' between chamber headspace and liquid flowing through its base... The jump from eq. 2 to 3 is not obvious enough and requires investigation of two references. This is not justified and more detail should be given here. It would help to spell out δ -stem.

Results: More visual presentation of the results, e.g. means by species/range/frequency, will be good (only a table is presented). Mass flow, means advection? The discussion of the test of the assumption of no mass flow into the chamber is not clear. The argument of a negative $\delta^{18}\text{O}$ value in the chamber is a qualitative one and does not indicate that getting values of around -3 are not a net effect of diffusion and mass flow (leaks...). Testing the 1st assumption in the paper is critical for the discrimination estimates and therefore critical to discuss in more

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convincing way. Eq. 4, 5 should also be combined, it's a linear system and series of steps in line, each with its D... This will also highlight the duplication in labeling, such as C_i/C_a used in both equations for different parameters. In this paragraph it is stated that D of 14 permil can only be obtained with liquid diff in the inner box. But this is not accompanied by the detail demonstrating this. Table 1 gives no error estimate. This is not normally acceptable, and especially critical when assessing the discussion on the AOX aspects. . .

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